necessary to remark that the problem of the rise of sap is one of mechanics, in so far as concerns the mode of the flow and the propelling power.

Contrary to the view above referred to, it seems not unreasonable to consider that the weight of the sap in each vessel is sustained in the main by the walls and base of that vessel, instead of being transmitted through its osmotically porous base to the vessels beneath it, and thus accumulated as hydrostatic pressure.

We could in fact imagine, diagrammatically (as happens in ordinary osmotic arrangements), a vertical column of vessels, each provided, say, with a short vertical side-tube communicating with the open air, in which the pressure is adjusted from moment to moment, and yet such that the sap slowly travels by transpiration from each vessel to the one next above, through the porous partitions between them, provided there is an upward osmotic gradient, *i.e.* if the dissolved substances are maintained in greater concentration in the higher vessels.[1] This difference of density must be great enough, between adjacent vessels, to introduce osmotic pressure in excess of that required to balance the head of fluid in the length of the upper one, into which the water has to force its way. Thus, in comparing vessels at different levels, the sap must be more concentrated in the upper ones by amounts corresponding to osmotic pressure more than counteracting the total head due to difference of levels, in order that it may be able to rise. As osmotic pressure is comparable with gaseous pressure for the same density of the molecules of the dissolved substance, the concentration required on this view is considerable, though not very great.

Such a steady gradient of concentration could apparently, on the whole, become self-adjusting, through assistance from the vital stimuli of the plant, for concentration in the upper vessels is promoted by evaporation. Yet pressures in excess or defect of the normal atmospheric amount might at times accumulate locally, the latter giving rise to the bubbles observed in the vessels, through release of dissolved gases.

It may be that this assumes too much concentration of dissolved material in the sap, *as it exists inside the vessels of the stem,* to agree with fact. In that case the capillary suction exerted from the nearest leaf-surface might be brought into requisition, after the manner of Dixon and Joly, to assist in drawing off the excess of water from the vessels. The aim proposed in this note is not to explain how things happen, which is a matter for observation and experiment, but merely to support the position that nothing abnormal from the passive mechanical point of view need be involved in this or other vital phenomena.

As regards estimating the amount of flow, at first sight it may not appear obvious, *a priori,* that the transpiration through a porous partition or membrane, due to osmotic gradient, is equal or even comparable in amount to what would be produced, with pure water, by a hydrostatic pressure-head equal to the difference of the osmotic pressures on the two faces of the partition. But more exact consideration shows that, on the contrary, osmotic pressure is *defined* by this very equality;[2] it is that pressure-difference which would produce such an opposite percolation of water as would just balance the direct percolation due to the osmotic attraction of the salt-solution.

[1] Thus, in an ordinary osmotic experiment with a **U**-tube, the percolation of water through the plug gradually *produces* a difference of hydrostatic pressure on its two faces, which is *sustained* by the fixity of the plug itself, but would be at once neutralised if the plug were free to slide in the tube. This increase of volume of the salt-solution, by the percolation of pure water into it, is on the van 't Hoff analogy correlated with the free expansion of the molecules constituting a gas. It goes on with diminished speed under opposing pressure, until a definite neutralising pressure is reached, inaptly called the osmotic pressure of the molecules of the solute, which just stops it, while higher pressures would reverse it. The stoppage is due to the establishment of a balance between the amounts of water percolating one way under osmotic attraction, and the opposite way under hydrostatic pressure. The pressure established, *e.g.* in an organic cell immersed in salt-solution, is thus really the reaction which is set up against the osmotic process. That process itself is perhaps more directly and intelligibly described as the play of osmotic affinity or attraction, even though it must be counted as of the same nature as the affinity of a gas for a vacuum. *Cf. Proc. Camb. Phil. Soc.*, January, 1897, or Whetham's "Theory of Solution," p. 109.

[2] See preceding footnote.

It would, however, appear that the great resistance to flow offered by what botanists call Jamin-tubes, viz. thin liquid columns containing and carrying along numerous broad air-bubbles, is conditioned mainly by the viscosity of the fluid, and involves only indirectly the surface-tension of the bubbles. In fact, the resistance to flow may be expected to remain much the same if each bubble were replaced by a flat solid disc, nearly but not quite fitting the tube. Its high value arises from the circumstance that the mass of liquid between two discs moves on nearly as a solid block when the flow is steady, so that the viscous sliding has to take place in a thin layer close to the wall of the tube, and is on that account the more intense, and the friction against the tube the greater. The increased curvature of the upper capillary meniscus of the bubble is thus merely a gauge of the greater intensity of the viscous resistance instead of its cause, and modification of the surface-tension cannot be involved as a propelling power. The experimental numbers given by Dr. Ewart show that, even where the vessels are largely occupied by bubbles, the greater part of the resistance to active transpiration still resides in the partitions between them.

If the osmotic gradient, assisted possibly by capillary pull at the leaf-orifices, is insufficient to direct a current of transpiration upward, *capillary* alterations inside the vessels, arising from vitally controlled emission and absorption of material from the walls, cannot be invoked to assist: rather it must be *osmotic* alterations from one vessel to the next, of, so to speak, a peristaltic character, that might thus come into play. But any such alteration (of either kind) will involve local supply of energy. Is there a sufficient fund of energy, latent in the stem, to provide permanently the motive power for the elevation of the sap? In what form could this energy get transported there? The energies of the plant-economy come from the sunlight absorbed by the leaves. The natural view would appear to be that the work required to lift the sap is exerted at the place where the energy is received, and that it operates through extrusion of water by evaporative processes working against the osmotic attraction of the dissolved salts; while the maintenance of equilibrium along the vessels of the balanced osmotic column, with its semi-permeable partitions, demands that an equal amount of water must rise spontaneously to take the place of what is thus removed.

The subject might, perhaps, be further elucidated by observation of the manner in which the flow is first established at the beginning of the season, or possibly by experiments on the rate at which water would be absorbed by a wounded stem high above the ground.

EXPERIMENTS WITH THE LANGLEY AËRODROME.[1]

THE experiments undertaken by the Smithsonian Institution upon an aërodrome, or flying machine, capable of carrying a man have been suspended from lack of funds to repair defects in the launching apparatus without the machine ever having been in the air at all. As these experiments have been popularly, and of late repeatedly, represented as having failed on the contrary, because the aërodrome could not sustain itself in the air, I have decided to give this brief though late account, which may be accepted as the first authoritative statement of them.

It will be remembered that in 1896 wholly successful flights of between one-half and one mile by large steam-driven models, unsupported except by the mechanical effects of steam engines, had been made by me. In all these the machine was first launched into the air from "ways," somewhat as a ship is launched into the water, the machine resting on a car that ran forward on these ways, which fell down at the extremity of the car's motion, releasing the aërodrome for its free flight.

In the early part of 1898 the Board of Ordnance and Fortification of the War Department allotted 50,000 dollars for the development, construction, and test of a large aëro-

[1] Abridged from a paper by Dr. S. P. Langley in the Smithsonian Report for 1904.

drome, half of which sum was to be available immediately and the remainder when required.

The flying weight of the machine complete, with that of the aëronaut, was 830 pounds; its sustaining surface, 1040 square feet. It therefore was provided with slightly greater sustaining surface and materially greater relative horse-power than the model subsequently described which flew successfully. The brake horse-power of the engine was 52; the engine itself, without cooling water, or fuel, weighed approximately 1 kilogram to the horse-power. The entire power plant, including cooling water, carburettor, battery, &c., weighed materially less than 5 pounds to the horse-power. Engines for the large machine and for a model of the large machine one-fourth of its linear dimensions were completed before the close of 1901, and they were immediately put in their respective frames, and tests of them and of their power-transmission appliances were begun.

A test of the quarter-size model in actual flight was made on August 8, 1903, when the machine worked most satisfactorily, the launching apparatus, as always heretofore, performing perfectly, while the model, being launched directly into the face of the wind, flew directly ahead on an even keel. The balancing proved to be perfect, and the power, supporting surface, guiding, and equilibrium-preserving effects of the rudder also. The weight of the model was 58 pounds, its sustaining surface 66 square feet, and the horse-power from $2\frac{1}{2}$ to 3. This was the first time in history, so far as I know, that a successful flight of a mechanically sustained flying machine was made in public.

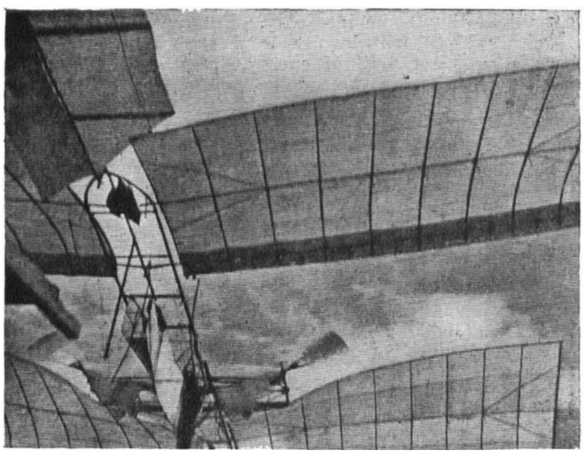

FIG. 1.—Reproduction of an instantaneous photograph, taken from the boat itself and hitherto unpublished, showing the aërodrome in motion before it had actually cleared the house boat. On the left is seen a portion of a beam, being a part of the falling ways in which the front wing was caught, while the front wing itself is seen twisted, showing that the accident was in progress before the aërodrome was free to fly.

Serious delays in the testing of the small machine were caused by changed atmospheric conditions, but they proved to be almost negligible compared with what was later experienced with the large one.

On October 7, 1903, the weather became sufficiently quiet for a test. In this, the first test, the engineer took his seat, the engine started with ease and was working without vibration at its full power of more than 50 horse, and the word being given to launch the machine, the car was released and the aërodrome sped along the track. Just as the machine left the track, those who were watching it, among whom were two representatives of the Board of Ordnance, noticed that the machine was jerked violently down at the front (being caught, as it subsequently appeared, by the falling ways) (Fig. 1), and under the full power of its engine was pulled into the water, carrying with it its engineer. When the aërodrome rose to the surface it was found that while the front sustaining surfaces had been broken by their impact with the water, yet the rear ones were comparatively uninjured. As soon as a full examination of the launching mechanism had been made, it was found that the front portion of the machine had caught on the launching car, and that the guy post, to which were fastened the guy wires which are the main strength of the front surfaces, had been bent to a fatal extent. The machine, then, had never been free in the air, but had been pulled down as stated.

On December 8, 1903, a test was made at Arsenal Point, quite near Washington, though the site was unfavourable. The engine being started and working most satisfactorily, the order was given by the engineer to release the machine, but just as it was leaving the track another disaster, again due to the launching ways, occurred. This time the rear of the machine, in some way still unexplained, was caught by a portion of the launching car, which caused the rear sustaining surfaces to break, leaving the rear entirely without support, and it came down almost vertically into the water.

Entirely erroneous impressions have been given by the account of these experiments in the public Press, from which they have been judged, even by experts, the impression being that the machine could not sustain itself in flight. It seems proper, then, to emphasise and to reiterate, with the view of what has just been said, that the machine has never had a chance to fly at all, but that the failure occurred on its launching ways; and the question of its ability to fly is consequently, as yet, an untried one.

There have, then, been no failures so far as the actual test of the flying capacity of the machine is concerned, for it has never been free in the air at all. The failure of the financial means for continuing these expensive experiments has left the question of their result where it stood before they were undertaken, except that it has been demonstrated that engines can be built, as they have been, of little more than one-half the weight that was assigned as the possible minimum by the best builders of France and Germany; that the frame can be made strong enough to carry these engines, and that, so far as any possible prevision can extend, another flight would be successful if the launching were successful; for in this, and in this alone, so far as is known, all the trouble has come.

The experiments have also given necessary information about this launching. They have shown that the method which succeeded perfectly on a smaller scale is insufficient on a larger one, and they have indicated that it is desirable that the launching should take place nearer the surface of the water, either from a track upon the shore or from a house boat large enough to enable the apparatus to be launched at any time with the wings extended and perhaps with wings independent of support from guys. But the construction of this new launching apparatus would involve further considerable expenditures that there are no present means to meet; and this, and this alone, is the cause of their apparent failure.

Failure in the aërodrome itself or its engines there has been none; and it is believed that it is at the moment of success, and when the engineering problems have been solved, that a lack of means has prevented a continuance of the work.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The number of first-year students matriculated on Saturday, October 21, was 1008. Last year at the same date the number was 884. With those matriculated during the Lent and Easter terms, the total for the civil year 1905 is 1039; but this number will be slightly increased, as several freshmen were unable to attend on Saturday. Hitherto the largest entry has been 1027, in the year 1890. The number of medical students is 117; there is also a large entry of engineering students and of candidates for the economics tripos.

The professor of mineralogy has, with the consent of the Vice-Chancellor, re-appointed Mr. A. Hutchinson, of Pembroke College, to be demonstrator in mineralogy and assistant curator for five years from January 1, 1906.

The special board for biology and geology has nominated Mr. F. A. Potts, of Trinity Hall, to use the university table at Naples for six months as from October 1, 1905.